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THE JOURNAL OF PHILOSOPHY

RELEVANCE AND SCIENTIFIC METHOD

A. S. EDDINGTON'S charming reduction of "An elephant slides down a grassy hillside . . ." to a set of pointer-readings[1] illustrates strikingly the abstract character of the goal and method of science. Eddington correctly asserts that science can not possibly be *all-comprehensive,* can not exhaust reality (though I think he is decidedly in error when he claims that science can never get at the *intrinsic* nature of the real objects it measures). But Eddington leaves his readers (at least he leaves *me*) with the impression that this abstractness and partiality of science is a defect. Hence I beg leave to quote at length an equally charming parable of the absurdity of eschewing all abstraction. It is found in A. D. Ritchie's *Scientific Method.*

"Once long ago (the precise date escapes my memory) a new librarian was appointed to a certain library. One morning it occurred to him to examine the catalogue. After some study he turned to an assistant who chanced to be present, and said: 'I do not think I like this book very much. It is a fallacious and, in a sense, inaccurate document, and what information it contains is trivial. I see nothing said about any book except the author's name, the title, the size of the book, and trifles of that sort. Everything mentioned in here is to be found in the book itself, and much more beside that is of greater interest. What is put down is correct enough, I suppose, as far as it goes; but to my mind "suppressio veri" is just as misleading as "suggestio falsi"; worse, in fact, because the deception is subtler. Suppose now, we abolish the catalogue—' 'Oh, sir,' interrupted the assistant, 'the catalogue is indispensable—it gives the class-marks of the books.' 'So I have observed,' replied the librarian with scorn; 'and they likewise are to be found on the books.' 'But how,' wailed the assistant, 'but how are readers to find the books?' 'As they do now,' the librarian said, fixing the assistant with a look, as of the stony basilisk, 'by looking on the shelves!' The assistant fainted. The librarian, who was really quite a kindly man, had him taken out into the fresh air where he slowly recovered.

[1] *The Nature of the Physical World,* pp. 251 f.

In the meanwhile the catalogue was burnt. All these events occurred a long time ago, as I have mentioned."[2]

I wish to assert that scientific knowledge is always abstract, and that it is not by that fact any the less valid knowledge. I wish, furthermore, to investigate what sort of ground is necessary to substantiate the claim that abstract knowledge can be completely and ultimately valid.

Abstract knowledge always tacitly involves the claim that not every factor in the universe is equally relevant to any given one. To claim that *a* is essentially relevant to *b* is to claim that you can not have true knowledge of *b* without knowledge of *a*. Hence if one were to assert that everything in the universe is essentially relevant to *b*, one would have to admit that *b* can be truly known only if everything in the universe is known, i.e., that an abstract knowledge of *b*, omitting even one of its relational properties, is impossible.

The sort of knowledge science seeks is abstract. Inevitably, then, scientific thought and method must reveal the assumption that some things or characteristics[3] are irrelevant to others. Scientific method proceeds by seeking the relevant, by disdaining irrelevant factors by the cartload.[4] That scientific method does assume that not everything is relevant to everything else and that it does concern itself with a search for the relevant alone may perhaps be made clearer by a brief investigation of certain processes and concepts of vital importance in scientific procedure.

First, consider scientific observation and the concept of fact. It is a commonplace that the good observer must be one who knows what to look for, who has been properly trained in the confining of his attention to certain factors only in the total situation. The view that science starts by merely collecting facts, then classifying them, and finally theorizing about them is a caricature if it means that facts lie around loose, that sense experience comes perfectly articu-

[2] *Op. cit.*, pp. 111–112.

[3] I use "factor" to designate either a thing (existent) or characteristic (property).

[4] Morris Cohen, in his *Reason and Nature*, does not tire of reiterating this perhaps obvious yet nevertheless fundamental truth. I select as representative a passage where he is discussing imagination as a supposed opponent of reason. "But all imagination, poetic as well as scientific, is essentially selective, in the sense that it concentrates upon what is relevant to the task before us. Such studied selection in the interest of what is relevant takes place in scientific observation, in trained memory, and generally in all the processes of scientific knowledge, so that the growth of genuine scientific knowledge is itself a progress of the imagination, not in falsifying the order of fact, but in eliminating insignificant, accidental, and irrelevant elements" (p. 70).

lated into members having equal scientific importance.[5] Scientific observation is selective. I think we can even say that a scientific fact is itself a selection, a product of selective observation. A fossil, a chemical reaction, a coefficient of friction, so far as they are elements in scientific knowledge (and only so far can they be of significance in scientific method), are abstractions. They omit, as irrelevant, hosts of factors truly present in the immediate experience of the scientist observing them. Immediate experience in its total concreteness is not abstract, but it is not, as such, a fact for science.[6]

When we turn to the process of scientific mensuration, the selectivity, the disregard of the irrelevant, is even more apparent. Norman Campbell points out that only those properties for which there are rules closely analogous to the basic axioms of arithmetic ("rules of counting") are measurable. He says, "If a property is to be measurable it must be such that (1) two objects which are the same in respect of that property as some third object are the same as each other; (2) by adding objects successively we must be able to make a standard series one member of which will be the same in respect of the property as any other object we want to measure; (3) equals added to equals produce equal sums." By way of illustration he uses the property weight. "Weight is measured by the balance. Two bodies are judged to have the same weight if, when they are placed in opposite pans, neither tends to sink; and two bodies are added in respect of weight when they are both placed on the same pan of the balance. With these definitions of equality and addition, it is found that the three rules are obeyed."[7] Science has been particularly ingenious and fortunate in devising operational rules of dealing with certain experienced properties such that addition, subtraction, equating, and other fundamental mathematical processes can be given empirical meaning relative to these properties. But obviously science is here selecting and disregarding. Within the totality of characteristics of a general sort of property which is itself measurable, such as weight or length, only a few characteristics are relevant to the rules for measurement of that property. For example, the rule for adding lengths is to place them end to end in a straight line. But this is obviously a selective principle, forcing the measurer to disregard all sorts of other combinations of distances which actually are characteristic of the thing measured. Furthermore, there are many general properties of things for which rules

[5] Poincaré's first chapter in *Science et méthode* is on "Le choix des faits," and he begins it by urging, "Nous ne pouvons connaître *tous* les faits, puisque leur nombre est pratiquement infini. Il faut choisir. . ." (p. 7).

[6] *Cf.* Cohen, *op. cit.*, ch. iii, §1, on "Reason and the Gathering of Facts."

[7] *What is Science*, p. 117.

analogous to the axioms of arithmetic have not yet been formulated and perhaps never can be. These properties (including perhaps all of the so-called secondary and tertiary qualities) are disregarded by the scientist so far as his method is one of measurement.[8]

In experimentation the scientific assumption that not everything is relevant to everything is so obvious that elucidation is an absurdity. An experiment is a process of observation under *controlled* conditions. To control a condition means to eliminate it, to make it invariant while other conditions are varied, or to vary it independently of the variations of other conditions. Each of these methods assumes that at least some other conditions actually occurrent in the universe are irrelevant to the controlled condition. Every experiment aims to discover the relevance or dependence of a phenomenon upon certain actual conditions as distinguished from other actual, but irrelevant, conditions. It is a process of determining which of the possibly important relations investigated really are important. Moreover, before the experiment starts, the experimenter must have chosen a group of such relations to be tested—i.e., he must assume as the basis of his experiment that a host of actual relations are irrelevant; and I am inclined to think that no experimental science ever has, can, or should experimentally validate this assumption completely in the case of even one of its experiments. Galileo rightly disregarded the phase of the moon and what he had eaten for breakfast when he rolled his ball down his inclined plane.

The concepts of cause and scientific law are likewise based on the assumption that some factors are irrelevant to others. The ideal scientific law includes *all* relevant conditions and yet *only* relevant conditions, and states precisely *how* they are relevant. As Cohen aptly states it: "Every mechanical or physical law asserts that a certain phenomenon or characteristic depends on one or a few factors and on nothing else. The acceleration of gravitation depends upon mass and distance. Everything else is indifferent to it. The freezing of water depends on temperature and pressure, and nothing else is relevant. To hold seriously to the popular dictum that everything is connected with everything else would make the scientific search for determinate connection meaningless."[9] Perhaps no actual

[8] It is clear from the above brief statement that I do not agree with Eddington's view that measurements are mere symbols of something outside them and intrinsically unknowable by means of them (*cf. The Nature of the Physical World*, pp. 259 ff.). A mensurational property is just as truly a characteristic of the thing measured as is any other of its characteristics, and the thing itself is nothing other than the unique togetherness of all its characteristics. See "Numerical and Qualitative Identity," to appear soon in *The Monist*.

[9] *Op. cit.*, p. 151.

scientific law, even in mechanics, fully achieves this segregation of the sheep from the goats, of the relevant from the irrelevant. But that this is the ideal is clearly seen by the fact that laws are scientifically acceptable to the degree to which they approximate it. And what is here true of law holds also of the concept of cause. The doctrine that the total antecedent universe is the cause of any given event is certainly not a doctrine acceptable to science. In seeking causes science is seeking restricted, distinguishable factors within the universe. When the astronomer asserts that the cause of a solar eclipse is the interposition of the moon between the sun and the earth, he surely does *not* mean to include in the assigned cause the fact that Columbus discovered America in 1492; the latter is causally relevant to certain other events, but not to the solar eclipse. The very principle of the uniformity of nature itself, though apparently applying indiscriminately to all factors, really supposes that there are irrelevancies in the universe. If it means anything at all, it at least implies that some sequences of events are approximately repeated; i.e., it implies that the time and place (to say nothing of further characteristics of the total context) in which the sequence occurs are irrelevant to the sequence; the occurrence of the sequence does not depend upon them.

Finally, scientific theories and hypotheses involve selection and the tacit claim that relevance is a limited matter. If this were not so, there could not be *rival* theories of the *same* phenomena, the theories would be simply supplementary. The molecular theory of heat could displace the theory of a heat fluid only if it asserted a relation between heat phenomena and a possible cause which was considered to be not only different from, but incompatible with, that asserted by the adherents of the theory of a heat fluid. An hypothesis or theory is taken to be established or at least to be more probable than a rival to the extent to which it is borne out not indefinitely and in general, but by *relevant* observations and experiments; and these latter themselves, as we have seen, are selective. Furthermore, no theory is scientifically good unless it accounts for a limited, specifiable group of facts. If it accounts for other possible and actual facts just as well as those within its proper field, it is then not considered a good explanation of the latter. For example, capricious desire growing out of a sense of power might be an excellent theory to account for the whimsical commands of a tribal chieftain, but when extended to the inconstancy of the weather and the food supply, it becomes, scientifically, a very bad theory. I do not deny that a good theory may have much wider application than it is first thought to have, but this is because more facts are discovered to have characteristics relevant to it, not because it eliminates or softens the

distinction between relevant and irrelevant characteristics. There is no such thing as a scientifically good theory of all facts taken indiscriminately in all their characteristics.

I have no doubt spent too much time elucidating the obvious; but it is well to be absolutely clear that scientific method is throughout dominated by the assumption that some factors in the universe are irrelevant to others. Now of course it may be contended that this very assumption vitiates scientific results or makes them only provisional. But I can see no *reason* for such an objection (unless the pious *faith* that everything is equally relevant to everything else is a reason). Furthermore, not only the success of science but also the fact that *all* knowledge as truly (though not as explicitly) rests on the same assumption as does science furnishes a pretty good reason for claiming that the above-mentioned objection is wrong . So without further ado I turn to the following question. How can the scientific assumption that some factors only are relevant to any given one be justified? What right has science to select, to confine its attention, to claim that its admittedly partial and abstract knowledge is true?

As a first type of answer to this question let us consider traditional mechanics. It claims that science (or at least physics) has a right to select and investigate a few factors out of the total content of experience because only these few are real (or are physically real). These are the so-called "primary qualities." As Professor Burtt, in his *Metaphysical Foundations of Modern Physical Science,* has well shown, these primary qualities thus selected by the early modern physicists were chosen primarily because of their mathematical character. Modern physics grew out of the conviction that the real world is mathematically simple; it hence cast every factor in experience not of this character into the limbo of a merely subjective and ghostly existence. But such a "bifurcation of nature," as Whitehead has shown, is untenable. Epistemologically it gives rise to a radical dualism whose inevitable conclusion, if logically thought out, is absolute skepticism—which is, of course, inconsistent with the claim to know *any* of the real characteristics of the physical world. But the following objection is perhaps more significant for our present purpose. Let us pass by the assumption that the only principle of relevance in the physical world is that of mathematical simplicity of connection. However we define what is relevant and what is irrelevant, does the mere fact that we have shown something to be irrelevant for physical science prove that it is unreal (or at least not physically real)? Odors and sounds and tastes may be wholly irrelevant to what physics studies—that fact alone does not show that they have no physical reality. Physics

deals with abstractions; it can not justify itself by claiming that its abstractions are really not abstract, that they exhaust the total character of their physically real objects. We must, to borrow another phrase of Whitehead's, avoid the fallacy of "misplaced concreteness." In fact, I think we can safely accept the principle that everything experienced, just as it is experienced, is to be taken as real (and as physically real) until it is shown to be inconsistent with some other factor in experience likewise taken to be real. What the scientist correctly disregards as irrelevant can not thereby be correctly denied reality.

A second type of answer to the question we are considering is really a revolt against the one just mentioned. It may in general be called empirical or anti-metaphysical. It claims that everything experienced is equally real (or unreal, if you prefer), and that hence the principle of relevance is not determined by reality, but simply by the method and purpose of the scientist. This view has many forms, two of which I wish to consider, though they tend to pass into each other. They are positivism and pragmatism. The positivist says, essentially, that no two factors in experience are inherently any more or any less relevant than any other two. Relevance becomes wholly a matter of statistical correlation, i.e., relative frequency together in experience. Hence scientific method is essentially a process of formulating briefly or in summary those types of sequence or coexistence whose frequency in experience is at least relatively high; it disregards as irrelevant to any phenomenon all other phenomena whose correlation with the given one is low. Thus, for example, Karl Pearson says: "The universe is made up of innumerable entities, each probably individual, each probably non-permanent; all man can achieve is to classify by measurement or observation of characteristics these entities into classes of *like* individuals. Within these classes variation can be noted, and the fundamental problem of science is to discover how the variation in one class is correlated with or contingent on the variation in a second class." [10]

Before going further we must note certain difficulties in this positivistic view. In the first place, it talks about *like* individuals; it denies that any two experienced particulars are absolutely the same. But how does science judge whether particulars are sufficiently alike to be properly classed together? Surely not by considering one by one *all* the characteristics they possess.[11] Rather, *some only* of their characteristics are here used by science, viz., those considered im-

[10] *The Grammar of Science*, 3d edition, p. 165.

[11] Pearson himself says that particulars classified together "are not absolutely [the] same, because they really depend for sameness on an infinity of characters, only a very small number of which are or can in actual practice be examined and identified" (*op. cit.*, p. 158).

portant or relevant. Hence *before* the process of correlation can get started, a judgment assuming difference in relevance must be made. Furthermore, the positivistic interpretation fails to justify the myriad cases where science, without an investigation of the frequency of correlation with the matter at hand, disregards multitudes of actual relations as irrelevant. For example, on the positivistic view science could not claim that everything but pressure and temperature is irrelevant to the freezing of pure water until it had investigated the correlation of the freezing of water with *every* other factor in experience. This would probably be an endless, and it certainly would be a puerile, undertaking. Finally, we should note that science is not concerned merely with what we have actually experienced, but also with possibilities which may, or perhaps can, never be experienced. It tells us confidently what *would* happen *if* certain conditions, and these only, were fulfilled. On the law of entropy and the supposal that no new factor producing difference of energy levels will arise, the scientist talks about a state of the universe which not only has never been experienced but probably has never occurred, viz., where all energy is equally distributed throughout space.

Positivism easily passes over into pragmatism. It does not stop with the claim that relevance is a matter of frequency of togetherness in experience. It goes on to say that this frequency is made the principle of relevance precisely because it satisfies the purposes of the scientist, because it is convenient to him, is economical of his time, or serves as a universal means helping in the attainment of any sort of purpose. Thus Pearson says, "The object served by the discovery of [scientific] laws is the economy of thought; the suitable association of conceptions drawn from stored sense-impressions, permits the fitting exertion to follow with the minimum of thought upon the receipt of an immediate sense-impression."[12] Mach asserts that "Physics is experience arranged in economical order."[13] The laws and concepts of physics, according to Mach, are economical in that they save the trouble of having direct experience of all cases of a phenomenon and because they unburden the memory. In Professor Burtt this pragmatic element appears in a slightly different form. He claims that science seeks regular relations in experience because such knowledge is valuable in the pursuit of any other end. "Why is the need which science seeks to meet more fundamental than any other? Because, in brief, the aim of science is to establish dependable knowledge of the relations of things, and such dependable

[12] *Ibid.*, p. 78.

[13] *Popular Scientific Lectures*, tr. by McCormick, p. 197.

knowledge is needed in the effort to satisfy any other need."[14] "The ideal of science is to establish what is needed for the successful attainment of any other ideal."[15]

To the general pragmatic contention that scientific relevance is a mere matter of convenience or utility to the scientist or to man in general, I wish to object on the score that such a view would make science in its very basis subjective. I have shown that science seeks essentially to discover and formulate just what is relevant to what, and how it is relevant. But if all relevance is merely a matter of utility, then the scientist is telling us not about real things, but about his (or society's) purposes. I certainly believe it *is* useful to know just in what ways various factors depend upon others; but it is useful only in so far as it is *so*—the usefulness does not create, but rather presupposes, the dependence. "The economic efficiency of scientific knowledge is based on something in the facts."[16] Shorthand is very useful and economical *if* there is something to be said; otherwise it is merely an odd pastime for people who haven't much to do. If some factors were not *really* much more important than others, science could not summarize experience; it could only give excerpts from it. And the contention that all relevance is relevance for some purpose is false unless one such purpose is simply to discover what factors are really relevant to any given one. Selection of means for a purpose itself presupposes that the real world is so constituted that only certain factors are relevant to a proposed end; and though what particular end we seek may be determined wholly by us, once we have chosen a definite end, the relevance of proposed means is determined by the nature of the objectively real world.

I hold that traditional mechanics is correct in claiming that the principle of relevance has ontological validity; what is relevant to what is not a mere matter of convenience. But I hold with the positivists that the relevant has no peculiar right to preëmpt reality or existence to itself. I contend that the prosaic and common-sense view, to the effect that the real world is neither thoroughly relevant nor thoroughly irrelevant throughout, but that some factors in relation to a given one are much more relevant than others, is a correct view. I hold the astonishingly conservative opinion that science does try to tell us about real things, and that if anything whatever which it tells us is true, then, since everything it says is abstract, we must admit that real things themselves possess unessential, irrelevant characteristics.

More particularly, I believe that a real thing or existent is

[14] *Religion in an Age of Science*, p. 24.
[15] *Ibid.*, p. 48.
[16] *Reason and Nature*, p. 153.

simply the unique togetherness of all its characteristics or attributes. But some of these characteristics belong to it merely as a matter of fact; they are not part of, nor implied by, its essential nature. They are irrelevant but actual characteristics of the thing; the thing would be just *what* it is were they different, and hence, though they are predicable of the thing, the thing is independent of them.[17] For example, Galileo's pendulum did have the relational property of being in motion at a particular time in a particular place, but this was a mere detail. Galileo could rightly disregard such properties as irrelevant (though he could not properly deny their existence) in stating such an essential characteristic as the following, that the period of oscillation of the pendulum was invariant despite variations in the amplitude of the arc described. Every existent thing has hosts of characteristics which are included in it as existent, but can be properly disregarded when we are concerned with *what* it is that exists. Science is concerned with essential natures, focal characteristics, not indiscriminately with every matter of fact.

EVERETT W. HALL.

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A FUNDAMENTAL DIFFERENCE BETWEEN THE NATURAL AND SOCIAL SCIENCES[1]

IT is to be expected that during the early stages of the development of the social sciences there should be many varied and conflicting beliefs about the relation between them and the natural sciences. At one extreme are those who believe that no science of anything social is possible or even desirable, those to whom the prospect of analysis and all of the rest which goes with scientific inquiry is essentially repugnant. At the other we find those who delight in exactness and rigorous formulation, but who find nothing distinctive in the social and who hope to achieve its reduction to the physical. To these materialists, who hold that the social is only the physical in disguise, there is no *special* problem of the relation of these two great classes of science: for to them the question of the relation of the social to the natural is just one aspect of the more general question of the relation of non-physical appearance to physical reality. In this discussion I mean to avoid both of these extremes: taking it as obvious that there are organized bodies of

[17] For a further exposition of my point of view, see "Focalized Identity," to appear soon in *The Monist*.

[1] Read, with minor omissions, at the Thirtieth Annual Meeting of the American Philosophical Association, Eastern Division, Charlottesville, Va., December 28, 29, and 30, 1930.

knowledge whose subject-matter is the social and which deserve the name of science, and, against the extreme materialism, pointing to the fact that as yet the social sciences have not been shown to be derivable logically from the natural.

However, within this middle position there is little agreement. In the first place, we are not sure whether all the studies dealing with social phenomena exhibit enough of the characteristics of science to be fairly called sciences; for example, the questions, "Is history a science?" and "Are the historians trying to find the laws of historical events?" have no answers that may be called the settled opinion of students within the field, and, consequently, there may be said to be no agreement whether history possesses even the aim of science, or whether, granting perhaps that it has the aim, its achievements make it worthy of the title. Secondly, there is the controversy about the status of values in reference to the social sciences: we have those who believe that the progress of the social sciences rests upon the possibility of the complete exclusion of all consideration of value, and those who believe that value is part of the proper domain of these sciences. Thirdly, there is the great difference of opinion about the kind of methods to be employed. The mere volume of this controversy led to Poincaré's devastating remark that, while the physicists are concerned almost wholly with results, the sociologists are concerned almost wholly with methods. Other great issues might be mentioned, but it seems as if the more one surveys the field the more he is apt to come to the conclusion that every possible combination of belief has its representative, and no combination more than one.

No one would deny the importance of these issues, nor that their solution is necessary for the progress of the social sciences and of philosophy. We should come to some conclusion about the boundaries of the social sciences, about the possibility of including elements of value in their data and in their generalizations, and about the methods that are proper or peculiar to them. Yet in face of the reigning chaos it may be well to begin afresh, to ask ourselves if there is not some simple distinction which has been neglected, which is significant enough to be worthy of statement, and relevant enough to those more prominent issues to give some promise of helping us toward their solution.

My thesis is that this simple relation is to be found in the kind of law or theory peculiar to each kind of science: that the complete formulation of any true proposition from social science always includes as part of itself some reference to the content and extent of some definite belief, while the complete formulation of any true proposition from natural science never includes any such reference

to belief. This distinction in kind of proposition may also be expressed as follows: assuming that a true proposition is the assertion that certain conditions are connected with certain other conditions, if the proposition is from social science one of the conditions is a belief, while if the proposition is from natural science none of the conditions is a belief. The exact form of the belief condition is "if persons 1, 2, 3, etc., believe *a*, *b*, *c*, etc." For each law of social science this form would have to be given a content; that is, what is believed, and who believes it would have to be stated as part of the law. Of course, this belief condition is meant to include the cases of disbelief and of absence of belief.

There are three main arguments in support of this distinction. The first is general and derived from the logical structure of any science. The second is more empirical and based upon actual cases. The third may appear as sophistry.

One of the general characteristics of a science is its internal organization. We commonly, and quite rightly, define a science as an organized body of knowledge, and this means that no one proposition stands alone, but rather that it is set in a context of other propositions and itself constitutes part of their context. This systematic structure of the fields of knowledge properly called sciences is very familiar and needs no emphasis. But I would simply point out that the context includes conditions under which alone any one of the propositions is true. Such conditions are strictly necessary. Usually in our rough and ready speech and thought, we forget about these necessary conditions, especially when the effects of their variation are negligible or when there is little likelihood that they may fail to hold in the expected course of events. Many illustrations might be cited to show the presence of these necessary conditions and our habitual omission of any reference to them, and one simple example will suffice. Is it true that water boils at two hundred and twelve degrees Fahrenheit? Yes, but only under the conditions of normal atmospheric pressure and under all the other conditions that only a physicist could state properly. Ordinarily we attach no qualifications at all to our answer, because great variations in pressure do not occur frequently, and the effect of slight variations is not important to us. Practically, what we do in this case we do in all: state only those necessary conditions which are liable to sometimes hold and to sometimes fail when the holding or failing makes a difference that is significant. But the complete formulation of any proposition would include them all.

This general argument does not show any difference between the natural and social sciences, but it does show where we should look to find the difference when we come to specific cases. Let us take

several examples from economics. Here we find a curious phrase, often used, and all too vaguely used, to cover all the unstated and perhaps unknown conditions upon which the truth of any law depends, namely, the phrase, "other things being equal." Such a general qualification needs analysis. For instance, an increased supply leads to a decrease in price, other things being equal, and I hope to show that one of those other things is the belief of those whose activities are generalized into this law. If, as a buyer, I know that several different prices are being asked for a commodity, I will probably buy at the lowest. If I do not know this, there is no telling which of the prices I will pay, and the same applies to all buyers. And, if a seller knows that a competitor is asking less, or that he can not get rid of his supply at the current price, he will probably reduce his price too. Again, if the seller does not know that by lowering the price he will stimulate the demand, there is no telling what he will do. In fact, the connection between the two conditions which the law asserts is very far from being a mechanical connection, but is brought about by the knowledge possessed by the buyers and sellers. The complete formulation of the law would then include at least the following: "If the supply is increased, and if both buyers and sellers are aware of the situation thereby created, and if other things are equal, then prices decrease." It is obvious that we do not need to state this belief—or, if you prefer, knowledge—condition in connection with the law of supply because it holds true so regularly.

But let us take another case. Suppose that a statistical study results in the generalization that six months after a rise in the price of commodity *a* the price of commodity *b* will rise also, other things being equal. If this law becomes known to those concerned with the two commodities, there will be an increased demand for *b* and a decreased supply of *b* whenever the price of *a* advances, both due to the expectation of the increase predicted. This all results in an increased price for *b* now rather than six months from now. In other words, one of the conditions of the truth of the generalization was ignorance of the generalization itself, and the knowledge of it tends, not to make it false, but to make it inapplicable, because the condition under which it is true no longer holds. While in the former illustration it was unnecessary to state the belief condition because that condition is so uniformly satisfied, here it is necessary to state it because it is so likely to fail.

As a final but more telling illustration from economics, there may be cited the following fundamental proposition, or axiom, from a sketch of a deductive development of economics: "Man constantly seeks what he believes to be the greatest satisfaction of his wants and

those of his family by the means which he thinks call for the least effort."[2] Here the belief conditions is stated explicitly.

A fourth case is from psychology. Suppose a man has an excessive fear of large crowds. The psychologist seeks to find the cause in some early experience. The generalization involved, while frequently not stated exactly, is "if an early experience of such and such kind, then excessive fear of large crowds." When that early experience is discovered it is explained to the patient, and sometimes he is cured. That can only mean that the presence of the cause and ignorance of the causal relation lead to the presence of the effect, while knowledge of the relation leads to absence of the effect despite the presence of the cause. More exactly, it means that the cause is not merely the early experience, but is rather that early experience and ignorance. A large number of the generalizations of psychology are of this sort and the belief condition must be included in them.

Again, let us take the generalizations of the "apostles of despair," such as Oswald Spengler and Joseph Wood Krutch, who predict the decline of our civilization. Certainly, our own historical perspective must be admitted as historically new. The civilizations they study were ignorant of all the results of modern historical research. Perhaps this ignorance is necessary for the truth of their generalizations. At any rate, there is no showing that it is irrelevant until we get a test case, i.e., until we discover a dead civilization that knew what we know, or until something decisive happens to our own. If ignorance be admitted as a necessary condition of the generalizations upon which their prediction is based, it then follows that those generalizations do not apply to the present situation simply because not all the conditions are fulfilled. Thus anyone who rejects the prediction on the ground that our knowledge of the generalizations is precisely the thing which alone may prevent our civilization from declining is admitting the presence of an unstated belief condition as an essential part of those generalizations.

The negative aspect of the thesis, that no belief condition is part of any law from natural science, is more easily illustrated. Certainly, the planets have followed their courses independently of any belief of ours about them or about anything else, and likewise the increase in the number of people who have come to believe in the laws of astronomy has had no effect upon the sequence of the position of the planets. Or to revert to a former example, we should not expect to find what anyone believes to have any effect upon the temperature at which water boils.

[2] Rueff, Jaques: *From the Physical to the Social Sciences*, p. 108 (quoting C. Colson: *Cours d'Economie Politique*, Vol. I).

Paradoxical as it may appear, the social sciences are at once too much and too little dominated by the natural. Too much in that they, like the natural sciences, confine their generalizations to events which are independent of man's knowledge, and too little in that they have generally failed to follow the ideal of the natural sciences in including in their generalizations all necessary conditions. Certainly, the "classical" economics was based upon the acceptance of the Newtonian ideal, and one of the standard criticisms of those economic theories is that they are not applicable when men lack, among other things, knowledge of their own economic interests.[3] This criticism is the admission of a belief as a necessary condition for the generalizations of the classical economics.

This difference between the natural and the social sciences suggests that the processes of verification in the two are radically unlike. In the natural sciences the fact which verifies or disproves a generalization occurs independently of knowledge of the generalization (although, of course, the *use* of that fact as a *verification* is dependent upon knowing the generalization), while in the social sciences the occurrence of the fact is dependent upon the knowledge of the generalization. In other words, the process of verification in the social sciences is in part circular. A belief may give rise to the very kind of behavior which shows the belief to be true, or to the kind of behavior which shows the belief to be false, but in either case the verification or disproof is partly circular.

Let me guard against a possible misunderstanding. Undoubtedly, if anyone wished to explain how it was that men ever succeeded in arriving at any knowledge of astronomy, he would have to include in his explanation a statement of the dominating beliefs of the early astronomers. In other words, a belief condition is necessary in a proposition explaining how we have achieved a knowledge of natural laws. A proposition about natural law may contain a belief condition, but that is not the same thing as saying that the natural law itself contains a belief condition. One might expect any generalization covering the growth of our knowledge to contain a belief condition precisely because this growth of knowledge is a genuinely social phenomenon. The knowing may be dependent upon belief, but the natural laws which are known do not include beliefs.

There is another possible source of misunderstanding. We may apply our natural science, and thus by creating certain conditions cause their natural consequences to come into existence also. And it may be utterly impossible to explain such applications without reference to what men believe. But this application, again, is different from the natural science which is applied. In fact, it may

[3] Élie Halévy: *The Growth of Philosophic Radicalism*, Pt. I, Chs. I, III.

be said that all of the applications of science, whether natural or social, are part of the domain of social science precisely because belief is involved in any explanation or generalization of those applications.

When we look at natural science in the large, we must admit that as a social movement it is dominated by two main motives, one something like curiosity, wonder, desire to know and to understand, and the other by the belief that "knowledge is power." This power is power to do, to achieve, to create. Is it not rather absurd, then, to carry from the natural sciences the conception of natural law which presupposes that we can formulate the laws of what men do and achieve and create without including in those laws any reference to their beliefs? The hope is that knowledge is a condition of doing, and then we study the doings as if knowledge were irrelevant. Our hope and our practice when we try to generalize in the social field without admitting any belief condition into our generalizations thus appear to be inconsistent. Either the hope should be abandoned, or the belief condition admitted as part of the laws of social science. And, if we choose not to abandon hope, it is practically necessary to state the belief condition in addition to merely admitting it whenever the effects of its variation are large and whenever there is any probability that it may in fact vary.

To establish adequately the distinction would require the analysis of a far larger number of examples than I have the time or ability to present, and, without doubt, some examples of propositions which appear to belong to social science could be offered in which it would be rather difficult, if not impossible, to find any belief condition. But here—and this is the argument that may appear as sophistry—it may be questioned whether such propositions really do belong to the social science field. If the distinction be granted, it must not be taken to mean that no part of natural science is relevant to or made use of by the social sciences, or to mean that none of the propositions which the social sciences use are from natural science. Physics uses mathematics without thereby becoming mathematics or mathematics thereby becoming physics, and likewise some natural science generalizations may have their greatest theoretical use in social science and still retain their status as essentially of the natural science type, although we should be misled, unless we exercise considerable care, into calling them social in kind. Thus, for example, a number of propositions of psychology, which is a social science, probably belong to biology, which is a natural science. We may be mistaken in what we class as social.

Nor does it greatly matter—and this is the second and last part of the sophistry—whether this distinction in kind of proposition is

identical with the distinction, ordinarily made, whatever that may be, between the natural and the social. This is not an inquiry into the proper use of the term "social," but rather a plea for the recognition of the two types of proposition. Even if no two of us use the word "social" in precisely the same way, we can face precisely the same question whether there are these two main types of proposition.

The distinction, I hope, does not presuppose the adoption of any special metaphysical position. It is presented more as of methodological or ethical significance. If one can give a materialistic account of knowledge, then the social collapses ultimately into the natural, and, if one can give an idealistic account of reality, the collapse is in the other direction; if one holds that knowledge is only a specialized form of an omnipresent "taking account of," then the distinction is one of degree and not of kind.

The distinction has at least the merit of being fundamental. It is frequently stated that the social sciences differ from the natural in the greater difficulty of experiment, the non-recurrent character of their phenomena, the greater number of factors operating, the greater difficulty of determining the influence of each factor, etc. These distinctions are rather superficial, and good cases to the contrary can be cited for each one, e.g., certainly the inability to experiment upon meteorological phenomena does not mean that meteorology is a social science.

But the chief criticism of these usual distinctions is that they unfortunately suggest that whatever we can discover in the social field, once the difficulties are overcome, is in all other respects like what we find more easily in the natural field. Certainly our social progress has been delayed by the unwarranted inference that, since there are immutable laws of social phenomena, the social situation is as little modifiable as the natural. Recently there has been a growing recognition that perhaps the social situation is alterable, but this has unfortunately brought the counter suggestion that perhaps social laws are mutable. If mutable laws mean laws that are sometimes true and sometimes false, then such a social science would have need of astrologers and magicians to determine just when any given law is true and when false. We shall, on the other hand, continue to have immutable laws whenever those laws are properly stated, but the situation is alterable because a belief is part of a social law and a belief may change, not thereby rendering the law mutable, but making an immutable law inapplicable and making one immutable yield application to another equally immutable. "Economic laws do not disappear, but their application to economic situations is continually changing, as the physical factors that seemed to give them immutability are found modifiable in their economic bearing

by psychical factors which carry a creative spirit."[4] State the necessary belief conditions of social laws and those laws become more scientific in character and at the same time suggest the possibility rather than the impossibility of modifying the social situation.

This simple distinction is relevant to at least one of the more prominent issues mentioned at the beginning, and I shall merely suggest in what way. It gives a locus for the introduction of values into the data and generalizations of social science. While the belief condition disclosed in the examples was always one of knowledge or ignorance of some facts or generalizations, the belief condition need not be so limited, and a belief in the form of a hope, an aspiration, or a simple judgment of value would be necessary to some generalizations. Value may enter a generalization without thereby destroying its scientific character, provided it enter as the content of a belief of definite extent.

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THE PRINCIPLES OF SYMBOLIC LOGIC

I WELCOME Dr. Ushenko's Note on the meaning of propositions in this JOURNAL (Vol. XXVIII, p. 715). In it he seems to me to have done a much needed and quite original thing. Alone of the believers in the "modern" (that is, symbolic) branch of Formal Logic, he has attempted to defend it against the criticism directed against it, for the past thirty years, by pragmatic logicians. Until he wrote, I had feared that they were wholly absorbed in multiplying, and trying to understand, their own symbols, or that they were pursuing a Brer Rabbit policy as the only safe way of dealing with unanswerable objections. So Dr. Ushenko's effort deserves a hearty welcome—though a page and a half hardly seem to me worthy either of the dignity of the subject or of the gravity of the situation.

It is to be regretted also that he should have spent so much of his scanty space on psychological speculation about the probabilities of my death-bed repentance and conversion to what he regards as logical orthodoxy, and on restating so many of my objections to symbolism, as if they were objections to me; but all this does not cancel his transcendent merit, which is sensitiveness to the criticism of which symbolic logic stands in such need.

To foment this salutary process further, I will set forth briefly (and therefore dogmatically) the points I made in my *Personalist* article on "The Sacrifice of Barbara," before proceeding to consider

[4] J. A. Hobson: *Economics and Ethics*, p. 125.

Dr. Ushenko's defence: for my state of mind I will refer him merely to the fuller article in the January (1932) number of *Mind.*

I believed myself to have brought *nine* vital accusations against symbolic logic. I pointed out (1) that logic could not be treated as a science of pure form, because, if form could be got pure, it would be merely verbal, (2) that the notion of *validity* is in no wise a substitute for *truth,* (3) that "truth" in symbolic logic suffers from a systematic ambiguity, because the "truth" of judgments is so different from that of propositions, (4) that symbolic logic has no means of distinguishing truth from falsity *by their form,* nor (5) any means of determining the *meaning* of its propositions, and that it is idle to discuss truth until meaning has been ascertained; (6) that, moreover, it can not distinguish between plurality of senses and real ambiguity, and that in consequence its "propositions" become infinitely ambiguous (potentially); (7) that they are not *propositions* at all, but *propositional functions,* and indeed (8) that *propositions* are the fundamental figments of symbolic logic, and do not exist at all, while (9) a symbolic logic that really confined itself to pure form would have no content left whatever.

Of these nine points Dr. Ushenko has taken up only the sixth, and that apart from its connection with the fifth, and the seventh. The cure he prescribes for the constant variation in the meaning of the same verbal form in its various uses is a "logical convention" to take "the minimum meaning of all meanings which are associated with the given sentence." He admits that it may be difficult to determine this, but infers only that "it will sometimes remain an ideal." But surely if he would allow for the development of meanings by *future* use, he would not imagine that the meanings can ever all be predicted and summed. Hence his minimum meaning would dwindle to zero, and become *impossible* to determine. In any case, however, to multiply its conventions and fictions seems a curious cure for the defects of symbolic logic, and I fail to see either how "this ideal is logically important" or is an aid to "precision of thinking."

On the other hand it is part of *my* case that "a proposition being inanimate is not subject to growth"; that is just the trouble about it! For our actual thinking *does* grow; so vigorously indeed that, strictly speaking, *any* significant predication (when successful) always changes the meaning of the terms it employs.[1] But Formal Logic has perversely chosen to build on the fiction that the meaning of terms is (or ought to be) *fixed,* and to talk about propositions rather than judgments. So the proposition becomes a helpless formula, totally incapable of reproducing the features of living thought. It has acquired its meanings from *past* uses; but these do not protect

[1] Cf. *Logic for Use,* p. 205.

it against "willful modifications" at the hands of masters of language like Humpty Dumpty, who make words mean what they please. Is not the whole history of philosophy one long illustration of philosophic audacity in manipulating language, and does not experience show that philosophers frequently get away with their arbitrary modifications of *"the"* meaning of words and "propositions"? I can not admit, therefore, that Dr. Ushenko has succeeded in making symbolic logic in any way relevant to the procedures of our actual thinking.

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BOOK REVIEWS

The Platonic Tradition in Anglo-Saxon Philosophy: Studies in the History of Idealism in England and America. JOHN H. MUIRHEAD. New York: The Macmillan Company. 1931. 446 pp.

This book is both a valuable addition to *The Library of Philosophy* and a great disappointment.

That it is a valuable addition to the library for which its author has served as editor will be anticipated by all who are familiar with Professor Muirhead's long, intimate, and sympathetic acquaintance with philosophical traditions on both sides of the Atlantic. The *Studies* here presented, as the author himself recognizes, "make no pretence of being a history of English and American idealism" (p. 15). Some readers will regret that, in spite of Professor Muirhead's recent volume on Coleridge, no chapter in this new book sums up the author's wide knowledge of Coleridge in a treatment proportional to that devoted, for example, to Arthur Collier or Carlyle. Likewise Berkeley, though referred to abundantly, is given no systematic discussion. And other important names receive only incidental reference. But the studies which Professor Muirhead does offer are competently and urbanely done. From the period prior to the nineteenth century Cudworth, Norris, and Collier are chosen for treatment. Of nine chapters on nineteenth-century idealism in England and eight chapters on idealism in America, five are in each case given to tracing the development of Bradley and Royce respectively. More interest attaches, however, to some other chapters reprinted from *Mind* and the *Philosophical Review,* partly because the subject-matter is more fresh, partly because here Professor Muirhead writes with the ardor of a pioneer. "How Hegel Came to England" and "Hegelianism in America" are two such chapters. So is the chapter on Pierce which speaks of the identification of his philosophy with pragmatism as a "misapprehension" (p. 328) and

maintains that antagonism to Cartesianism led him to reach independently what was almost an Hegelian position; for though there is evidence that Professor Muirhead "misapprehends" pragmatism (at least Dewey's type of pragmatism), he at least makes apparent some frequently overlooked elements in Pierce's thought.

To call the book also a great disappointment may seem to betray a whim of the reviewer rather than a lack in the book. Actually the disappointment is objectively justified because the book utterly fails to fulfil the promise of its title. The title should have been what is its sub-title; for the book is a series of studies in idealism. The book throws no light whatever on questions which the title would lead any student of philosophy or literature to expect to find analyzed with care. For example, there are, as is well-known, many Platonic elements in Cudworth; but a systematic presentation of Cudworth, even when an estimate is added of the extent to which Cudworth grasped "what must always be the central point in a sound idealism" (p. 60), makes little contribution to our knowledge of the rôle of Platonism in seventeenth-century England. Professor Muirhead has only done some of the preliminary groundwork necessary to a proper treatment of the theme his title announces. He does not tell us what Cudworth knew of Plato, in what form and through what other writers (e.g. Plotinus and St. Augustine) his knowledge of Plato came to him, what Cudworth thought Platonism to be and why he thought so, what problems drove him to Platonic positions, what happened to Platonism when Cudworth utilized it, in conjunction with other elements elsewhere derived, for his attack on Hobbes, what form Platonism consequently took in Cudworth's philosophy, and what effects Cudworth's form of Platonism had on the course of so-called Platonic traditions in Anglo-Saxon philosophy. Some material towards an answering of some of these questions Professor Muirhead gives because he knows and summarizes Cudworth's philosophy; but a treatment of these questions he can not be said to furnish. And so with the other idealists whose doctrines Professor Muirhead goes on to treat.

In his conclusion on "What is Dead and What is Alive in Idealism" Professor Muirhead sums up "the Platonic tradition" under five points. One can readily recall passages in Plato which Professor Muirhead could quote to defend this brief analysis of what Platonism means to him. One is left to wonder whether Platonism meant the same five points to Cudworth, Bradley, Royce, *et al.* But not merely are these five points overshadowed by the longer analysis to which Professor Muirhead at once proceeds of Neo-Kantian and Neo-Hegelian "developments" of Platonism. Also these points are given in a detached study that is not woven into the

body of the book; they are given in what seems almost an afterthought. Professor Muirhead is amply justified and thoroughly sound in insisting in his preface that the treatment of British philosophy as a record of empiricism beginning in Bacon and Hobbes and Locke and continuing on to Mill and Spencer and Sidgwick is a partial and one-sided treatment of a much more rich and manifold complex of philosophical speculations; he has gone on to summarize some phases of British philosophy that are too often slighted. But without more analysis and criticism it is misleading to lable these phases the "Platonic tradition." Professor Muirhead quotes on his title page Professor Whitehead's remark that "the safest general characterisation of the European philosophical tradition is that it consists of a series of footnotes to Plato." The book that would measure the degree of truth in this amazingly hyperbolic judgment has yet to be written.

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Du Cheminement de la Pensée. EMILE MEYERSON. Paris: Félix Alcan. 1931. 3 vols. Pp. xxvii + 1036.

La Philosophie de M. Meyerson. M. GILLET. (Archives de Philosophie, vol. VIII, cahier iii.) Paris: Gabriel Beauchesne. 1931. Pp. 114.

That the interest in M. Meyerson's interpretation of science is growing finds evidence in the increasing number of articles about it. The latest addition to this list is M. Gillet's *La Philosophie de M. Meyerson,* which shows the harmony between the Meyersonian point and that of *la Théodicée catholique.* M. Gillet believes that the works of M. Meyerson fortify three familiar proofs for the existence of God, the proofs from contingency, degrees of perfection, and finalism,

M. Meyerson's own writings have, moreover, been increased this last summer by the addition of his lengthy and detailed interpretation of logic. Needless to say, this study has only strengthened the conclusions its author had already reached in his previous investigations.

It will be recalled that science, according to M. Meyerson, is not simply legalistic but also causalistic, and that when a causal relation between two events has been formulated, an identification has been made between the two events. His latest treatise shows how the process of identification functions in thinking itself. This is accomplished by tracing the progress of thought from the diversity and multiplicity of the data with which the thinker is presented to the identity and unity of the conclusion with which alone he is satisfied. Until an equation is reached, thought does not rest content and

when—as is the case of the second law of thermodynamics—the facts resist identification, the thinker indulges in elaborate speculations to explain the facts away. Yet it is impossible to reach a true identity and the "irrational" always subsists as a fact even if, like a political minority, it receives no adequate intellectual representation.

It is certain that the tendency of modern logicians is to insist that their assertions are tautological. This means that they must be symbolized as equations and that whenever one side occurs in a proposition, the other can be substituted for it. Though M. Meyerson distinguishes in places between psychology and logic, logic is still for him "the art of thinking" and he denies its tautological character. Thus $2+2=4$—which of course is not pure logic but will serve as an illustration here—is not a simple identity, but an identification, because on one side of the equation is the symbol for an operation which is missing on the other. There is therefore *discovery* revealed in this assertion and discovery means new knowledge. Logic thus, for M. Meyerson, is more than a set of conventions for the use of certain symbols; it is a means of organizing the knowledge one has acquired from experience.

Thus he believes that the *a posteriori* element can never be eliminated from mathematics and gives (pp. 402 ff.) one of his characteristically brilliant examples of how even a formula containing an imaginary number as an exponent may be brought back to simple manipulations of pebbles as its source. At the same time—and in regard to the same example (p. 376)—he points out an instance of a mathematical discovery which seemed shocking to its contemporaries for the simple reason that there had as yet appeared no application of it to experience. It may seem strange that knowledge derived from experience and which always contains an *a posteriori* element should appear to be entirely *a priori* and inapplicable to experience. M. Meyerson does not clarify this mystery.

Part of the trouble seems to lie in a failure to distinguish between rules and their application, part in a failure to make precise exactly what "derived from experience" means. The particular school of logicians whom M. Meyerson is attacking would maintain that they are discussing the use of symbols and not their application to experience. Such a discussion should cause no such surprise as that voiced in a quotation from Mr. Hardy (p. 739, n. 64), that his belief that $2+2=4$ is not founded on the *Principia Mathematica* of Whitehead and Russell. Did Whitehead and Russell ever pretend that they were discussing the psychology of belief or the genesis of any individual's belief? One can not avoid Mr. Sheffer's logocentric predicament so easily, granting that one should try to avoid it.

As for the second question, an assertion may be "derived from

experience'' in a comprehensible sense of that expression without necessarily corresponding to experience. *Alice in Wonderland* contains nothing whose origin was not in experience, but very little in it is in correspondence with facts. One can build up concepts not only by extensive abstraction, but by simple abstraction, by generalization, by direct contradiction, by combinations according to *a priori* or *a posteriori* formulas of simple or complex data of experience. One has, to be sure, to grant men the ability to think. But epistemologists might be charitable enough to do that.

The limits of this review do not permit the detailed analysis which alone could do justice to these three thick volumes. One must be satisfied with indicating briefly the general tenor of their argument. They contain no page that is not a stimulus to further reflection or a challenge to prejudice and are a worthy companion to the really great works which have preceded them.

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Personality and Reason. ROBERTA CRUTCHER. London: The Favil Press. 1931. ix + 178 pp.

Dr. Crutcher's primary interest is in practical psychology and in problems of personal and social adjustment. Her theoretical inquiry into the meaning of the term ''personality'' has been influenced in the main by the late Professor H. Wildon Carr, whose positions she combines with tenets of the personalistic tradition. Her principal contention is that personality, which is ''the most real thing of which we have any experience,'' ''does not refer to a collection of states, but to an active, organized whole.'' This organization is ''spiritual in kind,'' and this activity ''expresses itself in thinking and doing.''

The alleged identification of all existence with personal existence by Bowne is rejected as non-evolutionary. With an appeal to Leibniz and to current popular conceptions of energy in physics, another restriction of the real world is substituted; namely, to the world of individual organisms. ''The real world consists of active individuals and their activity''; matter, on the other hand, is ''phenomenal.'' With recourse to the Bergsonian theory of ''creative evolution'' as against ''mechanistic'' views of evolution (in which the author includes the theory of emergent evolution), persons are declared to be latecomers in the universe, standing at the top of a hierarchy of living beings. This series of beings, it is conceded, does not necessarily imply a highest being; yet Dr. Crutcher favors, in view of the problem of reconciling infinite perfection with the limitations of personality, either a Trinitarian conception of God, rationalized in Hegelian fashion, or preferably a finite, striving God de-

scribed as a personified *élan vital* and as the "living God" of the New Testament. Of personal immortality the author is perhaps a little more convinced than of a supreme person. She is all the more certain, however, of personal freedom, defining freedom of the will on the basis of self-consciousness and of the achievement of self-organization through the knowledge and use of values as standards of judgment.

The theoretical and the practical activities of persons are interpreted quite in the manner of Croce and utilized for a defense of the unity of personality. The coördination of these activities, moreover, is said to insure the freedom of the individual. Psychological classifications of traits of individuals are judged to be acceptable, provided they are brought into line with a "phenomenal" interpretation of the "spatial body" and with the twofold classification of activity as theoretical and practical, and provided they contribute to what Allport has called *A Study of the Undivided Person.* For personality, it is maintained, illustrates the "concrete universal" of Hegel, in embracing the three "moments" of universality, particularity, and individuality, though shorn of the all-inclusiveness of Hegel's Notion.

Thus the treatise, selecting for the most part such materials as reflect the interests of Professor Carr, endeavors to effect a synthesis of Leibnizian monadology, vitalism, neo-Hegelianism, and personalism, with a view to supplementing scientific psychology. It is none the less surprising, considering the thesis defended, that but one sentence is devoted to the views of William Stern and none to the views of Coe, the living Boston personalists, and others. The main opposition is to an absolute idealism of the Bradleyan type as well as to mechanism and naturalism. The theory as presented is based upon an obviously preferential definition of existence, with the consequence that sharp oppositions are set up between the static and the dynamic, the physical and the mental, scientific knowledge and personal values, etc., whereupon one contrary is deduced from the other as the "phenomenal" from the exclusively "real." Had the metaphysical honors been equally divided, it would seem that personal evaluations might have been given a different status without being wholly lost, and any felt need of reconciling the oppositions might have been met by raising the previous question concerning the nature of the oppositions themselves.

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JOURNALS AND NEW BOOKS

SCIENTIA. Vol. LII, N. CCXLV–9. Das Mathematische im Menschen: *M. Dehn.* The Origin of the Solar System. Part I: From the Chaldeans to Chamberlin and Moulton: *A. C. Gifford.* La seconde loi de la thermodynamique s'applique-t-elle à la matière vivante?: *V. Ružička.* Il concetto di materia e il materialismo indiano: *A. M. Pizzagalli.*

Archives de Philosophie. Vol. IX, Cahier II. Supplément Bibliographie, No. 2. Paris: Gabriel Beauchesne, 1932. 59 pp.

Brun, J. Lucien: Les Libertés Culturelles. Étude de Morale Sociale. (Archives de Philosophie, Vol. IX, Cahier II.) Paris: Gabriel Beauchesne. 1932. 134 pp.

Graff, Willem L.: Language and Languages. An Introduction to Linguistics. New York: D. Appleton & Co. 1932. xxiv + 487 pp. $4.00.

NOTES AND NEWS

We are glad to publish the following communication from Professor Anna Forbes Liddell, Florida State College for Women, with regard to the Cusanus Commission of the Heidelberg Academie der Wissenschaften:

If that important period in world history, the transition from Medievalism to the Renaissance, has seemed to lack a philosopher who expressed the change in the current of thought, whose theory preserved the old even as it issued into the new, it is not because the fifteenth century brought forth no great thinker, but because the writers of the histories of philosophy have failed to recognize and properly evaluate Nicholas of Cusa.

It seems scarcely credible that a philosopher of the first rank should have remained for nearly five hundred years neglected, and therefore it were not surprising if the revival of interest in the life and writings of the Cardinal which has taken place in Europe during the past fifteen years may have seemed to many as no more than the enthusiasm of a few specialists. But this interest has steadily increased, and with greater knowledge of his work has come a growing conviction of its importance.

The long neglect can be explained. Although Nicholas was an ardent and influential Churchman—Cardinal, Bishop, Papal Legate, —although both in his writings and in his official policy he strove constantly to promote the unity of the Church and was so staunch a supporter of Pope Eugene IV as to be called the Eugenian Hercules, there is much in his writings which to the orthodox of a later time must have appeared dangerous if not heretical. His insistance that since there is only one God and one truth, no religion can be alto-

gether false even though some are inadequate, his effort to harmonize the Koran with Christian teachings rather than to refute it by counter arguments, and his purely intellectual interpretation of the resurrection, heaven, and hell, could not maintain the approval of the Church. Furthermore, in discussing creation and the nature of the physical universe Cusanus often utters statements more radical than those which brought Giordano Bruno to his death, more revolutionary than those which Copernicus discreetly withheld or Galileo was forced to recant. Many of these statements: that the earth and all heavenly bodies move but none describes the orbit of a perfect circle, that the earth is not the center of the universe, but only appears so to us because it is our focal point (any other point of reference would be the center for an observer stationed there), and others of like kind, have been so frequently restated during the intervening centuries and so thoroughly demonstrated scientifically that they have lost their novelty. Others, as, for instance, that the universe is finite but boundless, seem to belong to the scientific theories of our own day. Nor is this essentially modern spirit found only in a few scattered phrases; it pervades his whole thought. Had Cusanus lived a century later, his works, and probably his own person, had scarcely escaped the flames. That his writings were not put upon the Index may be because the Church was unwilling to bring opprobium upon her former champion, or perhaps only because the works were not sufficiently popular to be regarded as a menace to orthodox faith. In any case, their study was not encouraged, and the oblivion into which they sank was more effective than a prohibition of their use.

Some studies and translations of his various books have been made, but usually the work has been incomplete and inexact. Having only partial knowledge of his theory, or failing to see him in proper relation to his time, his commentators have usually overestimated particular aspects of his thought; many have regarded him as just one more pious mystic. Such errors were not unnatural. Cusanus is not easy to understand; his basic principle is the unity of opposites, the implication of all complex variety in the simple identity of one infinite truth. "This however surpasses our whole understanding, which lacks the power to combine contradictions in its own principle by the way of reason" (*De Docta Ignorantia*, Book I, Chap. 5). As the meeting ground of Medieval faith and logic with Renaissance humanism and the advancement of science, his own philosophy provides an excellent example of his theory. Often errors occurred through defects in the source material studied. His works were rare and inaccessible, and unfortunately the text was often so corrupt as to pervert the original meaning. A scholarly critical edition has long been needed, but the enormity of

effort which such a work required seemed to delay its being undertaken.

Finally within these last few years the *Heidelberg Acadamie der Wissenschaften* assumed the task. The members of the Cusanus Commission of the Academy are: Ernst Hoffmann, Raymond Klibansky, Heinrich Liebmann, Friedrich Panzer, and Heinrich Rickert. Co-editors are, also, Ludwig Baur and Gerhard Kallen. The ultimate intention is to edit all of the works, but limitation of funds has necessitated limiting the edition for the present to the philosophical and political writings. These, in fourteen volumes, will be brought out between now and 1940, the five hundredth anniversary of the appearance of *De Docta Ignorantia.*

The actual labor of collecting materials has not been easy. The European Catalogue of Manuscripts is not complete, and in consequence it has often been necessary to take long journeys in order to examine manuscripts. With patient thoroughness of exact scholarship the various texts have been collected, identified, and collated. Besides providing a reliable latin text the new edition will be furnished with a double series of valuable notes; this will greatly facilitate the study of Cusanus in his proper historical relation. The first series traces his own source materials, not only verifying the citations which he actually made, but determining those places in which he, like many other Medieval writers, made use of an author without giving any reference to his source. These notes are of especial interest because Cusanus was not only learned in the teachings of the Scriptures and of the Fathers, he was one of the first German humanists and an enthusiastic collector of early manuscripts. There is evidence that he read both Hebrew and Greek. His knowledge of Plato was not derived merely through the Neo-Platonic tradition; he was acquainted with many of the Platonic Dialogues, especially the *Republic.* The second series of notes sets forth the most important instances in which Cusanus was cited in the works of later authors, thus affording specific evidence of his influence. It is noteworthy that the list includes the names of Bruno, Kepler, and da Vinci.

The first two volumes of the philosophical and political works are now ready: *De Docta Ignorantia,* edited by Ernest Hoffmann and Raymond Klibansky, and *Apologia Doctae Ignorantiae,* edited by Raymond Klibansky. Others to follow are: *De Coniecturis; De Filiatione Dei; De Genesi; Idiota de Sapientia* I & II, *De Mente, De Staticis Experimentis; De Visione Dei; De Pace Fidei; Cribratio Alchorani; De Ludo Globi; Compendium, De Possest; De Beryllo, De Dato Patris Luminum, De Quaerendo Deum; De Apice Theoriae; De Concordantia Catholica* (two volumes).[1]

[1] Felix Meiner, Verlagsbuchhandlung, Leipzig.